

THE ANALYST.

MAY, 1892.

PROCEEDINGS OF THE SOCIETY OF PUBLIC ANALYSTS.

A meeting of the Society was held on Wednesday, the 6th of April, the President being in the chair.

The minutes of the last meeting were read and confirmed.

The following gentlemen were proposed as members :—Mr. Percy Gerald Sanford, F.I.C., F.C.S., Analytical Chemist, 20, Cullum Street, Fenchurch Street, E.C. ; Mr. Harry Silvester, F.I.C., F.C.S., Analytical Chemist, Holyhead Road, Handsworth, Birmingham ; Mr. L. J. de Whalley, B.Sc. (London), Chemist in charge at Messrs. A. Lyle and Sons, Limited, Plaistow Wharf, Victoria Docks, E. ; 26, Park Place, Greenwich, S.E.

Mr. J. Shorthouse Tweedie was proposed by the Council as an Associate.

Mr. Arthur William Cooke, F.C.S., Analytical Chemist, Portland Cottage, Ings Road, Wakefield, was duly elected as a Member of the Society.

Mr. C. E. Cassal read a paper "On a recent Case of Butter Adulteration." A discussion followed, after which Mr. Dyer read the following paper :—

ON THE INAPPLICABILITY OF THE WERNER-SCHMID METHOD OF THE ANALYSIS OF CONDENSED MILKS.

BY BERNARD DYER, B.Sc., and E. H. ROBERTS.

BOILING cane-sugar with hydrochloric acid results in the production of caramelised matter which is soluble in ether.

Quantities of cane-sugar, varying from 0.6 to 0.8 gramme, were dissolved in each case in 10 c.c. of water, the solution being boiled with HCl, and treated in the Werner-Schmid tube exactly as in a milk analysis. Ether residues were obtained, varying from 0.018 to 0.024 gramme, equal to from 0.18 to 0.24 per cent., comparing the fluid with milk.

Milk sugar gives no such residue, not more than 0.002 gramme being obtained in six experiments like the foregoing, milk sugar being substituted for cane sugar.

Experiments with milk to which cane sugar had been added, in proportions varying from 5 to 10 per cent., gave apparent percentages of fat by the Werner-Schmid method, ranging from 0.12 to 0.32 per cent. higher than the actual percentages obtained by the Adams process. Taking into account the necessary dilution of condensed milk, such errors would be multiplied up to from 0.6 to 1.6 per cent.

The authors conclude that the Werner-Schmid method, which gives quite accurate results for unsweetened milk, is inapplicable for sweetened condensed milk.

They also find that the Adams process, the milk being sufficiently diluted, and 5 c.c. only of the solution being used, is satisfactory for determining the fat even in highly-skimmed condensed milk.

DISCUSSION.

Mr. A. W. Stokes said that he also had, from recent experiments, noticed that some caramel was extracted with the fat in the Werner-Schmid process as ordinarily conducted. Dry caramel Mr. Stokes believed to be practically insoluble in ether, but as ether takes up some water, partly in solution and partly mechanically suspended, and as caramel is freely soluble in water, it naturally follows that in the Schmid process some caramel is extracted with the fat in the case of a condensed milk. If, however, after drying and weighing this extract in a flask or beaker, this be washed out several times with ether, it will be found that the ether takes up no caramel. In re-weighing the flask and subtracting its present weight from the former, the true weight of the fat will be obtained.

The advantages of the Schmid process for condensed milk are that a comparatively large quantity of the milk can be taken for analysis, and errors due to high multiplication for percentages are avoided.

In Mr. Stokes' experience the paper-coil method, owing to the presence of so much sugar, does not give the whole of the fat actually present in a condensed milk. Figures of analysis bearing out these contentions were adduced.

Mr. H. Droop Richmond protested against Mr. Stokes putting down certain figures on the board and calling them fat estimated by the Adams process, when, as a matter of fact, the process used was a modification. The advantage of the Adams process over others was, that the finely-divided fibres of the paper caused separation between the watery parts of the milk and the other part; and if a greater quantity of milk was placed on the paper than could be absorbed this condition was not attained. It was an essential point in Adams' method that a quantity, not greater than could be all absorbed, should be put on the coil, otherwise no separation took place, and it was better than direct separation to use 10 c.c. to one coil. He was sorry that this point had not been more strongly insisted upon. Separation would not take place with the method as used by Mr. Stokes

and this accounted for his low results. He might say that he had made some experiments with condensed milk. He had extracted the fat both by the Adams' process and by precipitating the casein, and the results had been almost exactly alike, with a slight advantage to the Adams process.

Mr. Bernard Dyer said that his experience quite agreed with Mr. Richmond's. If 5 c.c. of fluid were distributed over a strip of paper of a certain size, it was a different thing from distributing over a like surface 10 c.c. of a more dilute fluid. If 10 c.c. of milk were placed on an ordinary "Adams" strip of paper it was made almost "sloppy"; but if 5 c.c. were put on, slowly and gradually, that quantity barely seemed to wet the whole paper. The "selective" action of the paper came into play, and caused a separation of the fat, which could be traced as the paper dried. Even highly-skimmed condensed milk could be accurately analysed in this way. But if 10 c.c. of the diluted milk were taken, the conditions were altogether altered, and the result was the production of a film of "toffee" which the ether could not permeate.

A RAPID AND ACCURATE METHOD OF DETERMINING FAT IN MILK.

BY HENRY LEFFMANN, M.D., AND WILLIAM BEAM, M.D.

AMONG the processes for the rapid determination of fat in milk, those employing centrifugal machines have been most satisfactory. A procedure devised by us has proved, in the experience of others as well as of ourselves, to be rapid, accurate, and economical. For the test, we employ a bottle holding about 30 c.c., the neck of which is graduated, so that a volume of $1\frac{1}{2}$ c.c. is divided into 86 equal parts. 15 c.c. of the milk being taken, each of these represents 0.1 per cent., by weight, of butter-fat. The test is made as follows:—

15 c.c. of the milk are put into the bottle, 3 c.c. of a mixture consisting of equal parts of fusel oil and strong hydrochloric acid are added, mixed, and strong sulphuric acid poured in slowly, with agitation, until the bottle is filled nearly to the neck. The liquid becomes hot, and the casein is completely dissolved, a dark reddish-brown solution being formed. The neck is filled to near the zero point with a hot mixture of sulphuric acid and water, and the bottle whirled in the centrifugal machine for from one to two minutes. Milk very poor in fat may require from three to four minutes. The volume of fat which rises in the neck can be read off directly, or by the use of a pair of dividers. The points of these are adjusted at the upper and lower limits of the column of fat, allowance being made for the meniscus, then, the dividers being placed so that one point coincides with the zero of the scale, the exact volume can be read off. If but one test is to be made, it will be necessary, of course, in order to preserve the balance of the arms of the machine, either to make duplicate mixtures, or to fill the other test-bottles with diluted sulphuric acid.

We give herewith the results of comparative tests with the Adams' method, care being taken that the paper used was free from any extractable matter. The bottles

employed in these tests were divided into 100 parts, and the figures here given were obtained by multiplying by the factor 0.86. (As noted above, we now use a bottle divided into 86 parts, and thus save calculation.) :—

WHOLE MILK.						
Sample.	Fat by Adams' method.			Fat by centrifugal method.		
No. 1	...	...	3.534	...	...	$\left\{ \begin{array}{l} 3.53 \\ 3.56 \\ 3.56 \end{array} \right.$
No. 2	...	...	3.90	...	...	$\left\{ \begin{array}{l} 3.95 \\ 3.99 \\ 3.95 \end{array} \right.$
No. 3	...	...	3.03	...	...	$\left\{ \begin{array}{l} 2.97 \\ 2.97 \\ 2.93 \end{array} \right.$
SKIMMED AND WATERED MILK.						
No. 4	...	...	0.56	...	...	$\left\{ \begin{array}{l} 0.51 \\ 0.51 \end{array} \right.$

Another determination was made of sample No. 4 by boiling down to one-half volume, and treating by the method, the results being calculated to the original volume. This gave 0.55 per cent. of fat.

Tests were made to determine the effect of longer rotation in the examination of milks poor in fat. Thus, by four minutes' whirling, the last sample gave, without previous concentration, 0.55 of fat.

Comparisons have also been made between the results of this method and the figures for fat as determined by calculation from the specific gravity and total solids (Hegner and Richmond's tables).

Sample.	Fat by calculation H. and R.			Fat by centrifugal method.		
No. 5	...	...	3.60	...	...	3.61
No. 6	...	...	4.15	...	...	4.10

We find, as a result of many tests, that with bottles accurately graduated, and reasonable care in manipulation, especially sufficient rotation, results will be obtained within 0.1 per cent. of those of the standard methods.

715, Walnut Street, Philadelphia, U.S.A.

This terminated the Proceedings of the Society.

THE AVERAGE COMPOSITION OF MILK.

By P. VIETH, PH.D.

Read at Meeting, April, 1892.

By the kindness and indulgence which the members of this Society have extended to me in such a great measure during the last ten years, I have been permitted from time to time to lay before the Society annual reports on the work carried on in the laboratory of

which I have been in charge since the year 1880. The most prominent, and perhaps also the most important figures—certainly the figures based upon the largest number of analyses—were those showing in monthly averages the composition of samples of milk as received from the places of production, *i.e.*, the farms situated in different parts of the country on which the cows were kept. The aim constantly kept in view was to examine as many samples as circumstances would permit, and, indeed, it has been the practice to ascertain the specific gravity of the contents of every single churn of milk arriving from the country, a test which, in the greatest majority of cases, would at once draw attention to any abnormality in the quality of the milk. The specific gravity was always corrected to 15°C. As to the analysis of the milk samples this had to be limited to the usual items, *viz.*, total solids, fat, and solids not fat. There has never a change been made with regard to the determination of total solids, for which 5 grms. of milk are taken and dried in a platinum capsule, first three hours on the steam bath and then three hours in the air bath at from 95 to 100°C. Fat was determined by means of the lacto-butyrometer up to July, 1884; since August, 1884 it has been calculated from specific gravity and total solids. Up to April, 1885, the formula used was that constructed by Fleischmann and Morgen; from May, 1885, to May, 1891, Fleischmann's corrected formula was employed; and since June, 1891, the formula of Hehner and Richmond. It is obvious, then, that results referring to fat—and also to solids not fat which were always taken by difference,—obtained at the various periods mentioned are not directly comparable. As I do not expect that another alteration in the manner in which the work is carried out will take place, or even if it should take place, that the results would be materially affected, I thought it desirable to re-calculate all the earlier results from the formula of Hehner and Richmond, and represent the corrected and strictly comparable results thus obtained in a diagrammatic form which shows the prominent points clearer than long columns of figures.

From the diagram it will be seen that the monthly averages varied between the following limits:—

Total Solids	...	...	...	12.4 to 13.6 per cent
Fat	...	...	...	3.6 „ 4.6 „ „
Solids not fat	...	...	...	8.6 „ 9.1 „ „

The average composition of the 120,540 samples examined during the last eleven years is—

Total Solids	...	...	...	12.9 per cent.
Fat	...	...	...	4.1 „ „
Solids not Fat	...	...	...	8.8 „ „

A single glance at the diagram reveals the well-known fact that the variations in the amount of total solids are mainly due to variations in the amount of fat. I say deliberately “mainly” and not “exclusively,” for looking more closely into the figures, we find that a larger percentage of total solids is, generally speaking, accompanied, not only by a rise in fat, but also, although to a very much smaller extent, by a rise in solids not fat.

The following figures will make this clear :—

12.5 total solids are accompanied on average by 8.70 solids not fat.

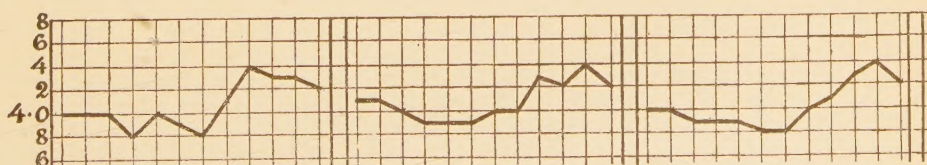
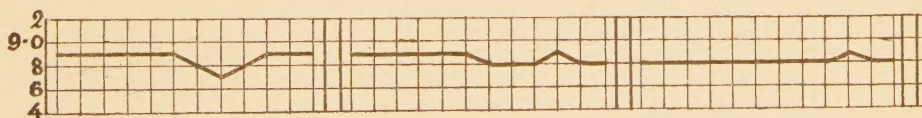
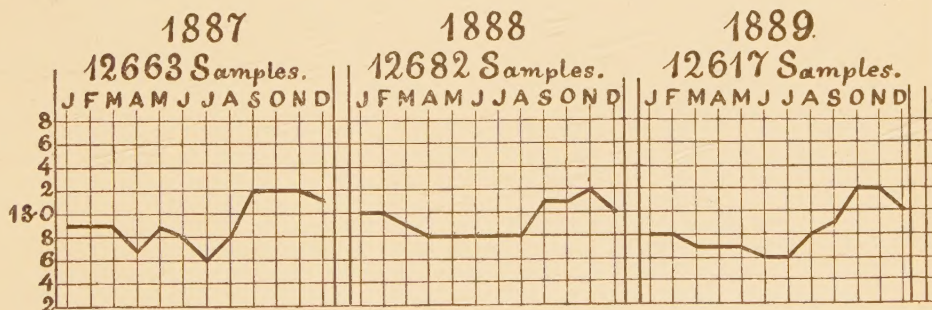
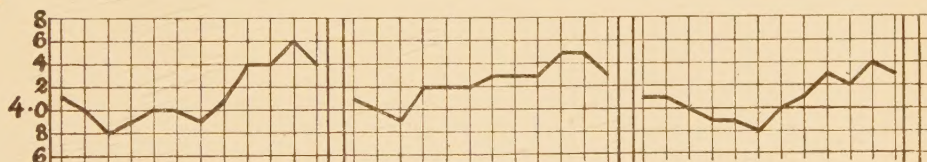
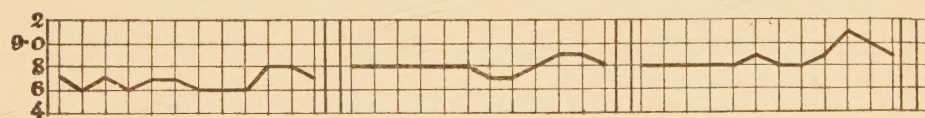
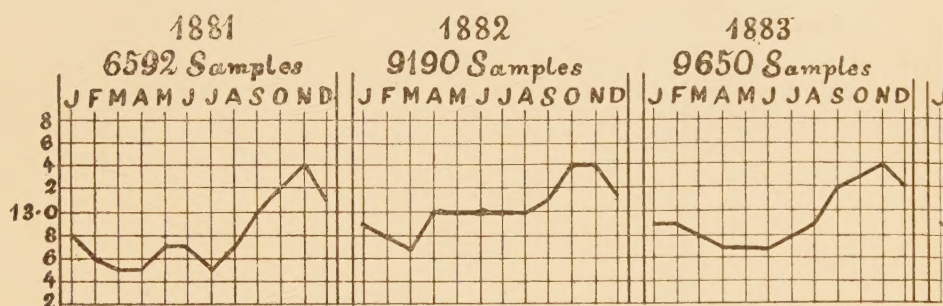
12.6	"	"	"	8.83	"
12.7	"	"	"	8.80	"
12.8	"	"	"	8.83	"
12.9	"	"	"	8.85	"
13.0	"	"	"	8.80	"
13.1	"	"	"	8.81	"
13.2	"	"	"	8.86	"
13.3	"	"	"	8.96	"
13.4	"	"	"	8.93	"

Broadly speaking then, a rich milk is distinguished from a poor, not only by a higher percentage of fat, but also by a somewhat larger amount of solids not fat.

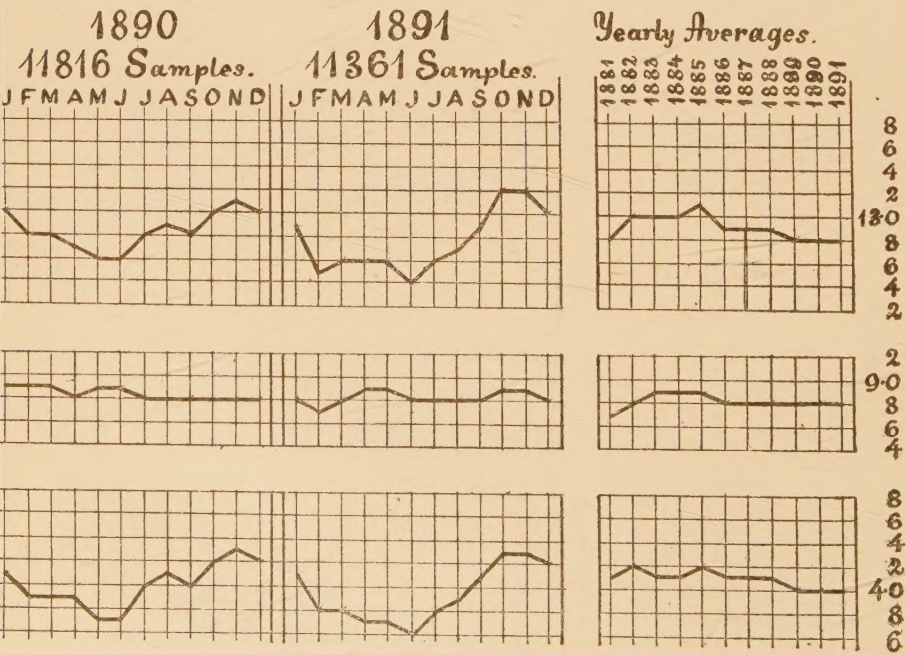
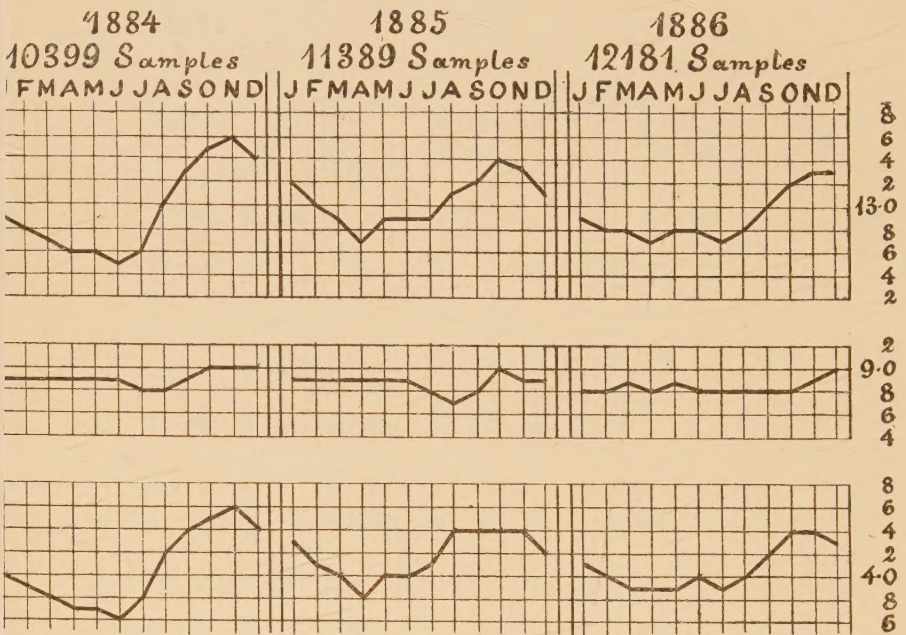
From the very large number of examinations on which the results are based, it might be inferred that the latter represent the actual average composition of the milk in question. This would undoubtedly be the case if there would be a strict relation between number of samples and quantity of milk. I shall better be able to make myself clear on this point by referring to the system on which the samples are taken. It is the rule to take at least one sample of milk per day from each supplier, morning and evening milk being sampled alternately; and further, on more or less frequent occasions samples from every single churn received from each of the suppliers on a particular day. The quantity of milk is taken into account so far that, as a rule, two samples are taken regularly in case a supplier sends more than five, and three samples in case he sends more than ten churns at a time. When the month is over, the averages are drawn of the analyses of morning milk and of evening milk from every supplier, and the mean between the two returned as the average monthly composition of the milk from that particular source. The average of the figures thus obtained represents the monthly average composition of all the milk received.

Is this mode of calculating the average composition fair—*i.e.*, can and does it give correct results? A first objection which might be raised is that morning milk and evening milk are treated exactly alike, notwithstanding the fact that they are by no means alike. Cows are milked, as a rule, between 5 and 7 o'clock in the morning, and between 4 and 6 o'clock in the afternoon. They give after the longer night interval a larger quantity of morning milk of a poorer, and after the shorter day interval a smaller quantity of evening milk of a richer description. The differences in quantity and quality may be considerable in case the intervals between the two milking times are more dissimilar. Here is an example: Twelve cows had, for some reason, to be milked during a period of ten days at 5 o'clock in the morning, and again at 12 o'clock mid-day. The first meal contained on average 12.15, the second 13.49 total solids; the yields stood in

Average Composition of 120,540



Samples of Milk.-- P. Vieth.



the proportion of 100 to 65. The mean percentage of total solids is 12·82, while the actual average, taking the quantities into account, is 12·68.

Under ordinary circumstances, however, the differences referred to are much smaller. The largest difference which I have observed with regard to milk received from a farm in the regular way, was as follows:—Morning milk 13·1, evening milk 14·1 total solids quantities in the proportion of 100 to 72. The mean of the total solids is 13·60, and the actual average 13·52.

Taking the milk all round, the differences are still less significant, as will be seen from the following figures referring to last year's milk:—

	TOTAL SOLIDS.		PROPORTION OF TOTAL SOLIDS.		PROPORTION OF QUANTITIES.	
	A.M. Meal.	P.M. Meal.	A.M. Meal.	P.M. Meal.	A.M. Meal.	P.M. Meal.
January ..	12·62	13·17	96 to 100		100 to 80	
February ..	12·27	12·73	97 „ 100		100 „ 83	
March ..	12·40	12·80	97 „ 100		100 „ 82	
April ..	12·42	12·78	97 „ 100		100 „ 81	
May ..	12·38	12·81	97 „ 100		100 „ 86	
June ..	12·21	12·58	97 „ 100		100 „ 91	
July ..	12·45	12·74	98 „ 100		100 „ 91	
August ..	12·47	12·92	97 „ 100		100 „ 84	
September..	12·68	13·11	97 „ 100		100 „ 84	
October ..	12·96	13·43	97 „ 100		100 „ 83	
November..	13·00	13·40	97 „ 100		100 „ 87	
December ..	12·73	13·27	96 „ 100		100 „ 85	
Yearly } average }	12·55	12·98	97 „ 100		100 „ 85	

The mean of the total solids is 12·765, the actual average 12·748. These last two figures prove that, for all practical purposes, one can neglect the difference in quantity between morning and evening meals when calculating the average composition of milk.

There is a further objection to the way in which the averages have been arrived at, viz., that every analysis does not represent an equal quantity of milk apart from the differences relating to morning and evening meals. Supposing there were a certain number of suppliers each sending one churn of milk of very high quality, and a similar number of suppliers each sending four churns of milk much inferior in quality. If one sample of the milk of each supplier were analysed, and the average of these analyses drawn, this would certainly not represent the average quality of all the milk mixed together.

For the last three years I have been able to compare and put before you the monthly average results, not only of samples taken from the railway churns on their arrival, but also of other samples taken from mixed bulks of the same milk, the latter samples representing the actual quality of the milk as sent out for distribution. The two series of samples

have shown slight differences on several occasions, and I have good reason to attribute these differences to the unavoidable neglect of the quantities of milk to which the analyses of the former series refer. The differences between the two series, whenever they did occur, were, however, only small; and as they practically disappeared in the yearly averages, the objection on this account may also be laid at rest, and the result, as stated, taken as representing the actual average composition of the milk to which the analyses refer.

So much with regard to the average composition of milk; and now, in addition, a few words with regard to limits, which may be not out of place, although, strictly speaking, they do not fall under the title of this paper. The year 1891 is distinguished by the fact that the quality of milk was lower than usual for fully six months, viz., from February to July. The yearly average is the lowest I have observed since 1880, and so is the monthly average for June. In June there were analysed 1,052 samples, taken from railway churns on their arrival. Among all these samples there was not a single one with solids not fat below the Society's limit, the lowest figure found being 8.70. In one sample the total solids fell below 11.5, viz., down to 11.38. As to fat, the results were less favourable. In fifty-one samples the calculated figures for fat were below 3 per cent., the lowest result being 2.6. As the worst samples came from a few particular farms, it was easy enough to keep the milk back from distribution and work it up for cream. I do not wish to be understood that results as low as some of those just mentioned, and perhaps lower, do not occur in other months; they are occasionally even met with during the time of the year when the highest average composition is observed.

My object in bringing these results under your notice is by no means to raise the cry that the standard adopted by the Society is too high. On the contrary, I think it very judiciously fixed. But in upholding the standard of purity it should not be forgotten that the cows have never been asked for, nor given their assent to it, and that they will at times produce milk below standard. A bad season for hay-making is, in my experience, almost invariably followed by a particularly low depression in the quality of milk towards the end of winter. Should the winter be of unusual severity and length—as was the case last year—the depression will be still more marked. Long spells of cold and wet, as well as of heat and drought during the time when cows are kept on pasture, also unfavourably influence the quality, and, I may add, quantity of milk.

I have on several previous occasions expressed my opinion, and should like to repeat it once more, that whenever the law should be altered, it would be highly desirable to word it so as to allow to condemn a milk, not “because it is adulterated,” but “because it is below a legally fixed standard.” This need not necessarily exclude prosecution in cases where adulteration can be proved, although the milk is still above standard.

The highest monthly average was, in 1891, as indeed in almost every year, that for November. In this month there were found, among 1,110 samples, forty-three in which

the three items, total solids, fat, and solids not fat, were respectively 14.0, 5.0, and 9.0 per cent. or above.

I have every now and again—more frequently this winter—come across samples with unusually high specific gravities which, to the superficial observer, would suggest abstraction of cream. Such samples have been received for a shorter or longer period of time from well-known dairies, which were absolutely beyond suspicion. Without going further into details, I will give you the complete analysis of one of the samples in question, the specific gravity of which was 1.0363:—

Water	...	...	...	...	...	86.14 per cent.
Fat	...	...	...	...	...	3.62 "
Proteids	...	...	...	...	...	4.66 "
Milk Sugar	...	...	...	...	...	4.58 "
Ash	...	...	...	...	...	.82 "

99.82

From this analysis it will be seen that proteids, and consequently also ash, are unusually high. My explanation is that the milk was yielded by highly-fed cows which were in the last stage of the period of lactation.

DISCUSSION.

Mr. Woosnam suggested that the members should have Dr. Vieth's table framed.

The President thought the suggestion an exceedingly good one. The table was a monument of patient labour and research. They had before them the results of Analysis of 120,000 milk samples, and were able to appreciate the amount of work that had entailed. There could be no doubt, that if it had not been for Dr. Vieth, Public Analysts would not have obtained their present position in regard to milk analysis.

REPORTS ON WATER ANALYSIS.

BY LEO TAYLOR.

Read at Meeting, March, 1892.

The question I have to bring before you this evening, follows almost naturally the paper read by my friend, Mr. Cassal, at the last meeting of the Society. Those of you who were present at that meeting will remember the severe manner in which several processes of water examination and analysis were dealt with. Generally speaking, the tendency of the discussion, I take it, was the upholding of the Society's methods of analysis. It is upon this point I wish to say a few words and ask the assistance of members in putting down, what I consider to be a most injurious practice, and one calculated to place analysts who have public duties to perform, in a false position.

At the outset let me disclaim any personal motives in this matter. I shall have to mention the name of a member of our profession who occupies a very prominent position,

who is many years my senior, and who probably practised water analysis when I was struggling with my alphabet; but I do so simply from motives of duty and because I feel that this course has been forced upon me by the gentleman I refer to.

It has been my privilege for eight or nine years past to act as chemist to the Walthamstow Local Board, and during that time I have analysed a large number of waters, principally from wells, the majority of which I have had to condemn, and the wells have been closed upon my certificate. In making these analyses, I have been guided by the scheme drawn up by the Special Water Committee of our Society, and published in *THE ANALYST* of July, 1881.

The details of my analyses comprised estimation of total-solids, solids non-volatile, solids organic and volatile, chlorine, nitrogen as nitrates, poisonous metals, hardness, oxygen reqd., saline and organic ammonia; physical examination as to appearance in 2-ft. tube, taste, smell, suspended matter and behaviour of solids on ignition. Upon the results of this examination I based my report and expressed an opinion as to the value of the water from a hygienic point of view.

In the early part of last year I was instructed to investigate the character of the East London Company's supply to Walthamstow, and it was found, during the seven months over which the sampling extended, that the quality of the water varied considerably in different parts of the town. Upon the quality of the samples it is not my intention to touch, as that has nothing to do with my subject to-night; but it was necessary for me to have gone thus far into detail in order that you may understand my position and the letters I am about to place before you.

The results of this investigation, together with my remarks upon each sample, were tabulated by the Medical Officer of Health of the district, and copies distributed to various authorities, amongst whom was Dr. E. Frankland, as official adviser to the Local Government Board.

The acknowledgment of Dr. Frankland's was in the following terms:—

THE YEWS, REIGATE HILL,
SURREY, *January 9th*, 1892.

DEAR SIR,

East London Company's Water.

I am in receipt of your letter of the 5th inst., and enclosure and in reply, have to say that the analyses given in the Report of the Medical Officer of Health are not complete. *They give no information regarding the quantities of organic elements in the samples; and failing these data it is impossible to form any trustworthy opinion as to the quality of the water from a hygienic point of view.*

I report monthly to the Local Government Board on the quality of the East London Waterworks Company's supply from (1) the river Lea, and (2) from deep wells in the chalk, the various wells being in turn submitted to investigation.

In December last, nearly all the river water delivered in London was of very inferior quality; but, *as a rule*, the river supply of the East London Company compares favorably with the waters derived from the Thames, whilst their supply from deep wells is invariably of excellent quality.

I am, DEAR SIR,

Yours truly,

E. FRANKLAND.

GILBERT HOUGHTON, ESQ.,

Clerk to the Walthamstow Local Board.

The statement contained in this letter was so extraordinary and far-reaching, imputing that all my water work for the Board had been valueless, and in fact opening up all sorts of legal questions as to the power of closing wells, &c., that the letter was submitted to me for consideration and report.

I replied to it as follows:—

BEACHFIELD, WALTHAMSTOW,

January 14th, 1892.

DEAR SIR.

I have seen the letter received by you from Dr. Frankland, and am surprised to notice in the first part of the letter Dr. Frankland states that my figures, as appearing in your Medical Officer's Report, are incomplete, inasmuch as the organic elements have not been determined. Dr. Frankland goes even further and says that "failing these data it is impossible to form any trustworthy opinion as to the quality of the water from a hygienic point of view."

I must emphatically traverse these statements absolutely, as entirely unwarranted.

Dr. Frankland has here committed himself to a statement which is utterly at variance with the opinions held by almost every chemist but himself.

Some years ago, the Society of Public Analysts appointed a Committee to investigate the question of Water Analysis, and a scheme was drawn up by that Committee, which has since been adopted by almost every analyst. The analyses I have had the honor to submit to your Board have all been worked upon the lines laid down by that scheme.

The combustion process of water analysis, or as it is more generally termed, the "Frankland" process, was invented by Dr. Frankland and has been tried and abandoned as too cumbersome and unwieldy for general use. Very few chemists even profess to use it.

This is not the time to enter into a discussion as to the relative value of the two processes, but I contend, and it has been proved beyond dispute, that either the "Frankland," or the "Wanklyn" process, in competent hands, will enable the same opinion as to the quality of a water to be arrived at. Therefore it is as wrong to say that a water cannot be judged from my figures, as it would be to say that Dr. Franklands'

analyses are incomplete and valueless, for want of the saline and organic ammonia determinations.

The latter part of Dr. Frankland's letter practically confirms my reports, the last sample which I examined being taken at the end of November.

The statement that "*as a rule* the river supply of the East London Company's compares favorably with the water derived from the Thames," goes for nothing, as he does not say that in December it *did* compare favourably; moreover, if it did, then it would only be equivalent to saying that the East London supply was the best of a very bad lot.

Acknowledging, as I do, the good qualities of the supply to the East side of the town, I feel that any samples to be examined must be taken without notice to the Water Company, because it would doubtless be a simple matter to supply the whole district with the better water while the samples were being taken.

In many of the districts served by the metropolitan Companies, the local authorities have instructed their analyst to periodically examine the water supply, as it has been found impossible to keep the Companies up to their duties without such supervision. The official analyses reported monthly (I refer to those signed by Messrs. Crookes, Odling and Tidy), are done *on behalf of the Companies*, and not for the Local Government Board, as is generally supposed, and do not therefore have the same value as independent analyses such as your Board instituted last year.

I remain, DEAR SIR,

Yours faithfully,

LEO TAYLOR.

GILBERT HOUGHTON, Esq.,

Clerk to the Walthamstow Local Board.

The Board was apparently satisfied with this reply, and wrote Dr. Frankland that the report on the water supply had been sent to him for his information only, and asking him if, in the discharge of his official duties, he would occasionally take East London Company's water from each end of the town and so embrace the different supplies.

To this Dr. Frankland replied, regretting that his arrangement with the Local Government Board did not allow his undertaking the collection and analysis of samples from the Walthamstow district, but he expressed his willingness to examine, under certain conditions, samples sent to him, and to include the reports on these in his monthly reports on the Metropolitan Water Supply. These conditions included the payment of a certain fee which need not be mentioned here.

There the matter rests at present. I do not fear that future samples will find their way to Reigate, instead of remaining in Walthamstow, but what I do fear is, that this suggestion of incompetence may make itself felt in other quarters where the analyst may not have the same confidence reposed in him that fortunately obtains in my case,

and therefore ask that some expression of opinion may be given upon this old and much debated question.

I have recently gone through much of the literature bearing upon the water fights in bygone years, and have been reminded of the exciting discussions, at some of which I was present, in this room. I do not anticipate that such times will come again, because I feel sure the almost unanimous feeling of analysts is against the organic carbon and nitrogen process, and it seems like raking up an old bogey to bring such a matter before you in the Year of Grace, 1892. The necessity for it, however, has arisen, and with a young member like myself, I could have wished a more prominent man should have brought the matter to your notice, but that in the nature of things is impossible, because I do not think the remarks against which I have been forced to protest would have been dared to have been made against a better-known chemist.

DISCUSSION ON MR. LEO TAYLOR'S WATER ANALYSIS.

Mr. Alfred H. Allen said he thought that Dr. Frankland had no right to force upon a brother analyst, who happened to be junior to him, or rather upon the clients who employed him, a particular method which he was perfectly well aware was not used by one analyst in a hundred. It had been well said that nothing more easily found its level than an analytical process. He was not prepared to suggest that it was justifiable to condemn a water only examined either by the Frankland or Wanklyn process, or any other process. He thought the whole of the results obtained should be taken into consideration. He thought it very important to have an estimation of the organic nitrogen, and any process which would do that was worth consideration.

Mr. Cassal thought there could be little doubt that if it were officially possible for Dr. Frankland to withdraw the letter referred to he would willingly do so. Dr. Frankland, as well as the analysts employed by the Water Companies, had, perhaps, been somewhat harried lately. There had been representations from different parts of London, pointing out the impure nature of the water supplies; several special samples had to be taken; and it was, perhaps, somewhat awkward that the analysts who made the first analyses, viz., those for the local authorities, and who had found that the waters were dirty, had to be agreed with in their conclusions. The sentence in Dr. Frankland's letter was certainly an unfortunate one, and it would be said by many that, from a professional point of view, it would have been far better if it had not been written. Dr. Frankland had said that Mr. Taylor's report gave no information concerning the quantity of organic elements, meaning thereby "organic carbon and nitrogen," in the samples, and failing these data, that it was impossible to certify upon them. It was quite impossible for Dr. Frankland to maintain such a position at the present day. Hardly any analysts in this or any other country had adopted the process used by Dr. Frankland. This matter again raised one of the points which he (Mr. Cassal) had brought before the Society at the last Meeting, and which had been, to a certain extent, discussed, viz., as to the advisability of giving

analytical details in reports on water. Mr. Taylor's case afforded an additional example of the evil resulting from the custom prevailing among analysts of giving analytical details in their reports. The question of water was a difficult one to deal with. It was doubtful, he thought, whether, in the case of water, the giving of analytical details could at present, and for some time to come, be avoided; but that it was originally wrong to do so, there could not be the slightest doubt. Mr. Taylor had simply followed the usual practice, and had given the results of the processes recommended by the Society. He, therefore agreed with the President in thinking that Mr. Taylor was entitled to the protection of the Society—at any rate to its official countenance—and, no doubt, it would be extended to him. He (Mr. Cassal) had, some time ago, condemned certain samples of water delivered by two of the London Companies as being dirty and insufficiently purified for a public supply. Dr. Tidy had taken the opposite view, and in the course of his report had referred to the analytical results. Dr. Tidy objected to the statement of results in parts per hundred thousand and parts per million, because he (Dr. Tidy) preferred grains per gallon, and took occasion to remark that a fraction of a "grain" was not worthy of consideration. That instanced the danger alluded to. Mr. Cassal held that the best course to adopt was simply to traverse such statements whenever they were made. He understood that Mr. Taylor had done this, and if so, his position was a strong one.

Mr. Bernard Dyer said that reference had been made by both Mr. Taylor and Mr. Cassal to the desirability of stating results in some uniform way; and reference had been made to the fact that the Society, some years ago, appointed a Water Committee which sat for a long time, and which drew up a scheme for the regulation of water analysis. He thought that it was a great pity that while some members adopted the processes for water analysis recommended by the Society, they nevertheless disregarded the Society's views as to the desirability of unanimity in the mode of stating results. Referring to a report which Mr. Taylor had laid on the table, he pointed out that, instead of stating all his results in grains per gallon, which was the way in which the Society of Public Analysts, in adopting the Water Committee's Report, suggested that they should be stated, Mr. Taylor (like, he regretted to say, many other analysts) put down his results in what he would forgive him for calling "a miserable hash up" of grains per gallon and parts per million. He thought it was highly desirable that, if figures were to be given at all, chemists should be agreed as to some definite way of recording them.

Mr. A. H. Allen suggested that the original report of the Water Committee, which had been referred to, should be reprinted by the Society at a price which would cover the cost; and that additional information, in regard to alternative processes, might be advantageously added.

Mr. Dyer, replying to Mr. Allen, said that enquiries had often been made for copies of the old water scheme, but it had been out of print for a long time. He thought it would be desirable to re-print the scheme. He had a very vivid recollection of the labours of the Water Committee, and he was not anxious to see another Water Committee

appointed to revise the scheme; but it might be possible to make some additions or suggestions. It would have to be carefully done.

Mr. W. Thomson thought that it would be advisable to reprint the pamphlet. He suggested that the Water Committee should be re-elected, and should re-consider the matter.

Mr. Cassal failed to see that, because some ten years ago, a committee of the Society of Public Analysts suggested that results of water analyses should be stated in grains per gallon, Mr. Dyer should wish to bind Mr. Taylor rigidly to the grain per gallon method. It was an obsolete method, and scientific men of the present day no longer used such measurements.

On the determination of Amyl Alcohol by Röse's method. A. Scala. (*Gazzetta Chimica Italiana*, 1891—346.)—The author reviews the previous investigations on the presence and amount of higher alcohols in fermented liquids, and gives a fairly complete list of the results of different observers.

In 1886, Röse (*Ber.* 1887—113), published a method of estimating the higher alcohols, founded on the property of chloroform to increase in volume directly as their quantity in a spirit. He employs a stoppered tube 20 m.m. in diameter, and 45 to 50 c.m. long, enlarged at the upper part, graduated from 20 to 45 c.c., and holding in all 175—180 c.c. Into this tube he measures exactly 20 c.c. of chloroform, previously purified and distilled, and 100 c.c. of the spirit to be examined which must contain 50 per cent. of alcohol by volume. The tube is closed and cooled to 15° C. in a vase of water for half-an-hour, then shaken vigorously for two minutes and replaced in the vase to allow the chloroform to settle; the tube being rotated between the fingers and gently shaken at the same time to assist the drops to descend. After a hour's repose at 15° C. the tube is read; 1 volume per cent. of amyl alcohol increases the volume of the chloroform by 2 c.c. The experimental error is 0.05 c.c.

Stutzer and Reitmar (*Centralb. f. allgm. Gesund. Hef. II.* 3, 191), modify the process by using 1 c.c. of sulphuric acid of 1.286 sp. gr., and by making their alcohol of 30 per cent. strength. They find that 1° C. of temperature makes a difference of 0.1 c.c. in the volume of the chloroform. Their mode of operation is as follows: Distil 200 c.c. of brandy or other spirit with several drops of caustic potash solution until four-fifths have passed over; make up the distillate to 200 c.c.; determine the specific gravity, calculate and add the quantity of water necessary to obtain a solution containing 30 per cent. by volume of alcohol; 100 c.c. of this alcoholic solution, 20 c.c. chloroform, and 1 c.c. of sulphuric acid are then treated as in Röse's method.

Sell (*Arbeiten. aus. d. Kais. Gesdh. Vol. IV.*, 1888), published a comparison of Röse's method with the two due to Traube (capillarimetric and stalagimetric), and gives the preference to the former; he gives two corrections that may be used in working,—1, a difference of 1° C. makes a difference in the volume of the chloroform of 0.26 c.c., and

2, an error of 1 per cent. by volume in the alcoholic solution makes a variation in the volume of the chloroform of 0.30 c.c.

Bardy (*Journ. Pharm. et Chimie.*, Vol. XVIII.), confirms Sell's preference, but uses a larger apparatus.

Fresenius (*Zeits. f. Anal. Ch.* 1890—307), expresses an opinion that spirits (cognac, rum, arrack) may contain a substance which has a contrary effect to amyl alcohol, *i.e.*, diminishes the increase of the volume of the chloroform.

In the author's experiments the alcohol used was the purest absolute alcohol of Kahlbaum, distilled from lime, and was brought to the exact density corresponding to 30 per cent. by volume in Hehner's tables: the chloroform was purified by Marquardt's method, *i.e.*, 200 c.c. of chloroform were heated with 3.5 gms. of potassium bichromate, 1.5 gms. sulphuric acid, and a little water in a closed bottle to 85° C. for 6 hours; the chloroform was then distilled, shaken with barium carbonate, being warmed at the same time of the water-bath, again distilled, dried with calcium chloride, and finally re-distilled ready for use.

The following are the details of manipulation:—Wash the tube with strong sulphuric acid, then with nitric acid, and finally well rinse with water to remove all acid; completely dry the apparatus by warming and blowing air through it and place it in a support. With a long-stemmed funnel pour in a little more than 20 c.c. of chloroform, and place the tube in a bath kept at 15° C. When the volume of the chloroform is constant, take out the excess with a long fine tube, and leave exactly 20 c.c. Place also in the bath the alcohol to be tested (at 30 per cent. volume strength) and some sulphuric acid of 1.286 sp. gr. Without removing the tube from the bath, measure in 100 c.c. of the alcohol, and 1 c.c. of sulphuric acid; stopper the tube, take out of the bath, invert and shake it about 120 times; again turn and place it in the bath to settle, gently rotating from time to time to assist the deposition of the drops clinging to the sides, and read the volume of of the chloroform after 20 minutes.

An apparatus graduated in $\frac{1}{20}$ c.c. is found to give the most exact results, it being possible to read to 0.01 c.c. If the graduation be finer, the bore is so narrow as to present inconveniences.

Base. By the *base* is meant the volume assumed by 20 c.c. of chloroform when shaken with 100 c.c. of the purest 30 per cent. by volume alcohol; this differs with various samples of chloroform, and should be determined for each; results in the paper are given showing from 21.35 c.c. to 21.65 c.c. for different chloroforms; with the same sample it is constant, and can be determined to 0.01 c.c.

The specific gravity of the alcohol is an important factor and must be determined accurately; a difference of ± 0.0001 makes an error in the reading of the volume of the chloroform of ∓ 0.0235 c.c.

A difference of temperature of 1° C. increases or diminishes the volume by 0.036 c.c.

Calculation of impurity. 1 c.c. of each of the following substances increases the volume of the chloroform over the base as follows:—

Aldehyde	...	...	0.200 c.c.
Paraldehyde	..	...	1.000 „
Furfurol	...	...	0.601 „
Propyl Alcohol		..	0.502 „
Normal butyl Alcohol			1.161 „
Iso—butyl	...	...	0.949 „
Amyl	...	...	1.507 „
Essence of Aniseed	...		0.588 „
„ „ Cinnamon			0.750 „

The author prefers to calculate the increase as amyl alcohol, because it is the most important constituents of fusel oil, and also because the error caused by other impurities is not likely to be very great owing to their smaller co-efficients.

He gives the following table:—

Increase in vol. of Chloroform.	Quantity of Amyl Alcohol.	Increase in vol. of Chloroform.	Quantity of Amyl Alcohol.
0.01 c.c.	0.0063 c.c.	0.1 c.c.	0.0634 c.c.
0.02 „	0.0127 „	0.2 „	0.1268 „
0.03 „	0.0190 „	0.3 „	0.1910 „
0.04 „	0.0254 „	0.4 „	0.2535 „
0.05 „	0.0317 „	0.5 „	0.3169 „
0.06 „	0.0380 „	0.6 „	0.3802 „
0.07 „	0.0444 „	0.7 „	0.4436 „
0.08 „	0.0507 „	0.8 „	0.5069 „
0.09 „	0.0571 „	0.9 „	0.5702 „
		1.0 „	0.6336 „

Determinations may be made with almost equal exactitude at temperatures other than 15° C. taking as *base* the volume of chloroform after shaking with pure alcohol at that temperature.

The author discusses the value of the results obtained, and shows that while alcohol (spirit of wine) contains usually less than 0.2 per cent. by volume of amyl alcohol, brandies contain over 2 per cent. by volume.

He concludes that the method of Röse is the most exact for the determination of amyl alcohol in spirits, but does not consider that the results of the determinations give much information. He hopes that in the future they may be more useful. H. D. R.

The Valuation of Commercial Aluminium. A. Rossel. (*Schweiz. Wochenschr. Pharm.* 1891, xxix. 471, through *Chem. Zeit.*)—The impurities present in commercial aluminum consist almost exclusively of silicon and iron. Both affect the value of the material considerably. The author reviews the usual methods which leave a good deal to be desired, and recommends the following process. About three to four grms. of aluminum are weighed out and dissolved in about 35 c.c. of warm caustic potash solution, containing 30–40 per cent. of KOH. A black flocculent residue is left. The solution is made acid with hydrochloric acid, without previous filtration and evaporated to complete dryness in a platinum basin. The residue is taken up with hydrochloric acid and the silica determined as usual. The iron is determined by dissolving a similar quantity by the same method, adding sulphuric acid in excess, and titrating with permanganate. Special attention should be paid to the necessity of having the caustic potash free from silica.

B.B.

The Weighting of Leather. B. Kohlmann. (*Chem. Zeit.* 1892, xvi. 16-17.) Samples of leather, the weight of which has been increased by illegitimate means, are by no means uncommon. As the price per kilo of a good leather is about three shillings, the practice is not unprofitable. Insoluble inorganic substances are seldom used for this purpose, because they, of necessity, remain on the surface and are easily recognised. The commonest adulterants appear to be sugar, barium salts, and worthless vegetable extracts. The literature on the subject is very scanty, and data must be compiled from the examination of genuine samples to determine the limits beyond which adulteration may be considered proved. All samples should be dried at 105° C., as the quantity of water varies between 12–20%. Experiments on pure leather showed that the dry substance contained 10–12% of matter soluble in water, and 0.5–1.0% of ash. Sugar and barium salts were not present. In testing leather it must be remembered that the excess of tannic acid, which is always present in good samples, yields sugar by the action of alkalis, and therefore an aqueous extract reduces an alkaline copper solution, such as Fehling's solution. The method adopted by the author consists in extracting a weighed portion of the sample, cut into thin strips, by repeated treatment with cold water, precipitating the tannin with lead acetate, removing the excess of lead with sodium carbonate, and determining the sugar in the filtrate by Fehling's solution, or by the use of the polarimeter.

The following table shows some of the results obtained :—

No. of Sample	...	1	2	3	4	5	6	7	8
Organic matter insoluble in water...	%	79.9	89.2	79.8	78.0	73.6	87.7	80.0	77.6
Organic matter soluble in water	...	15.9	10.2	15.8	20.9	22.6	11.8	17.1	19.3
Ash	...	4.2	0.6	4.4	1.1	3.8	0.5	2.9	3.1
Sugar	...	Present.	Nil.	Present.	Nil.	Present.	Nil.	Present.	Present
Barium	...	"	"	"	"	"	"	"	"

It therefore appears that only 2 and 6 are pure. Of the remainder, five contain both barium salts and sugar, and one (No. 4) is adulterated with soluble organic matter, while the ash is higher than is normal.

B. B.

Compressed Yeast. (*British and Foreign Confectioner.*, vol. xvii., p. 194.) Mr. J. Kirkland, in a lecture on Baking, delivered at the Heriot-Watt College, Edinburgh, states "For a long time it was considered impracticable to press yeast without the addition of starch, so that it would keep. This notion, which at best was but a superstition, is now exploded, and some of the large manufacturers have stopped making "mixed" altogether."

W. J. S.

Gravimetric Estimation of free Sulphuric Acid. M. Weinig. (*Zeit. f. angewand. Chemie.* 1892, p. 194). Free sulphuric acid can be determined in a similar manner to that proposed by Schaffgotsch for estimating free nitric acid, namely, by the addition of a slight excess of ammonia solution, evaporating, drying, and weighing the residue. The ammonium sulphate crystallizes water-free, melts at 140°C ., and decomposes at a higher temperature than this; it can therefore be safely dried at 115° to 120°C . For the preparation of a standard solution of sulphuric acid, a certain number of c.c. of of the diluted acid are pipetted into a tared platinum dish, about half a c.c. of pure ammonia solution added beyond the point of neutralisation, the solution evaporated to dryness, heated for half an hour to 120°C ., cooled in the exsiccator, and weighed. A simple calculation gives the amount of sulphuric acid contained in the quantity of dilute acid used in the experiment. The addition of too large an excess of ammonia is to be avoided, or spurting takes place during the subsequent evaporation.

W. J. S.

Soapy Milk. F. J. Herz. (*Mitt. Milchw. Vereins im Allgau*, 1892, vi. 9, through *Chem. Zeit.*). Samples of milk which were suspected of being skimmed on account of their high specific gravity (1.0351–1.0365 at 15°C .), were found to have a content of fat of 3.7–6.08%, while the specific gravity of the whey of the spontaneously curdled milk was 1.0313 in one instance, the fat of the same sample being 5.03%, and the solids not fat, calculated by Fleischmann's formula, 10.04%. In other cases, the values similarly arrived at were 10.05, 10.21, 10.25 and 10.26%. It therefore appears that Fleischmann's formula is inapplicable to these abnormal samples. The author adopts the term "soapy milk" for such samples because they possess a soapy smell, and maintain a persistent froth for as long as 24 hours. The smell is not apparent in the fresh milk, but is developed after it was stood 24–36 hours. On treating such milk with potash a fishy smell is produced, instead of the usual odour, recalling that of trimethylamine. The froth or scum on the milk is distinguished from that which forms on ordinary milk, in that it does not consist of tangible floating particles, and cannot be filtered off. "Soapy" milk shows

little tendency to curdle. Three samples, kept for 24 hours at 40° C. and then exposed to the ordinary temperature of the room, with free access of air and occasional shaking, took, in two cases, eight days and in the other twelve days, to curdle. Apart from its disagreeable taste and smell, "soapy" milk is troublesome to use in butter and cheese-making. The reason for this abnormality, which does not seem to depend on the period of lactation, and has been observed in well-fed cows, has not yet been detected.

B. B.

Drouot's Apparatus for the Examination of Butter. In 1887, F. Drouot (*Industrie Laitière*, 1887, 28, 285) introduced an apparatus for distinguishing artificial butters and mixtures from genuine butter.

The essential part is a little table of tin-plate 27 cms. by 7 cms. containing 6 hemispherical cups of about 32 m.m. stamped in it, a thicker iron plate of the same size with two handles at either end, and a spirit lamp to heat it.

This plate is first heated to about 60°C., and then placed over the little tin plate, in the cups of which are placed the samples to be examined (about 1.5 grains in each cup); the iron plate should be just so hot as to melt the samples in about 5 minutes, at the expiration of which time the aspect of the samples should be noticed. Genuine butter appears as a clear oil, with a layer of white liquid at the bottom, but margarine is always turbid, more or less opaque, and does not deposit a sharp coherent layer on the bottom. Mixtures give characters between the two.

The reflecting surface of the cups materially aids the observation.

Overheating is to be particularly avoided, as even undoubted margarine may thus become clear.

Besana (*Sui metodi atti a distinguere il burro artificiale dal burro naturale*. Lodi, 1888) gives a very favourable opinion of the method; all genuine samples gave clear liquids, except a very few butters obtained by shaking, which gave a very slight turbidity. Rancid butter cannot be tested by this method, unless great care be taken in removing the outer layers, and using only the centre portion.

Wollny (*Milch Zeitung*, 1887, 32, 658) confirms the author's statement that 10 per cent. can be detected by the Drouot apparatus, but Ravà (*Annuario d. R. Staz. Sperim. di Lodi*, 1887, 136) and Salvatori (*Staz. Sperim. Ital.*, 1888, xiv., 516) fix the limit of certainty at 20 per cent., and express themselves unfavourable to the apparatus for the detection of small mixtures.

The "Congress of Directors of the Italian Royal Agricultural Stations and Royal Agricultural Laboratories" recommends this method as a preliminary one, and the French "Society for the Encouragement of the Dairy Interests" awarded a gold medal to the author.

H. D. R.